CONFERENCE SUMMARY

James E. Gunn
Department of Astrophysical Sciences
Princeton University
Princeton, NJ 08544

I will attempt in this talk to address the questions that Drs. Schwarzschild and Faber posed at the beginning, in the light of what has been said in the last few days and a few things that were not but perhaps should have been.

1. DO WE REALLY NEED IT?

Do we really need dark matter to explain the world as we see it and presently understand it? The certainty of the "yes" answer to this question varies rather a lot depending on where it is that one thinks one needs it. We have heard conflicting views about the necessity for it in the solar neighborhood, but I think it is safe to say that there is very little remaining doubt about scales from the outer parts of galaxies on up. At the very largest scales the question is "how much", but even if Ω is as small as the dynamics of relatively well-understood structures will allow, there must still be a large amount compared to "luminous" matter, a loose term I shall use in this summary to mean the stars, gas, dust, remnants, and other objects we "understand". We might use "ordinary" or "baryonic", but those terms are predjudicial and probably not quite correct either.

It should be remarked at the outset that all that I shall say is predicated upon the physics we know and love being reasonably correct. Many of the phenomena which dark matter is invoked to explain can be, at least qualitatively, explained by some variation on Newtonian or Einsteinian gravitation, perhaps the best developed of which (but by no means the only one) is the theory of Milgrom which was described here.

Let us look briefly at the question of dark matter in the solar neighborhood, where the results discussed here by Bahcall seem to be in conflict with those of Kuzmin. It should be realized that the two determinations of the local mass density refer to two rather different populations, the Bahcall results on F dwarfs and K giants for a rather old, large scale-height system and the Kuzmin ones for a very young population confined very closely to the plane. It is thus not obvious that the discrepancy is real. Taken at face value, the results might suggest that the old disk population has something like half its total

J. Kormendy and G. R. Knapp (eds.), Dark Matter in the Universe, 537–546.

mass in unknown form, while we understand the young population reasonably well.

If we are forced to admit the existence of dark matter in the solar neighborhood and are led by economy of hypotheses to identify it with dark matter elsewhere, the consequences are striking. Bahcall has shown quite convincingly that its scale height cannot be more than double that of the old disk, which means that it currently occupies a very thin disk; there is little doubt that the bulk of it farther out is more-or-less spherically distributed, but if there is a concentration in a disk, it presumably must be dissipative and therefore presumably baryonic. Furthermore, since the bulk of the evidence suggests that the disk formed comparatively recently, the dark matter presumably formed out of gas in much the same manner as stars form.

We are not, of course, forced to assume that this stuff, if it exists, is the same stuff out of which halos are made and that accounts for the virial mass in great clusters, but if not we have two mysteries instead of one. It therefore seems prudent to ask how firmly one is required to believe that there is missing mass in the disk. I would here only like to issue a caution or two. Both of Bahcall's tracer populations have a potential problem brought about by the realities of stellar evolution. In the F dwarfs, slightly evolved subgiants which are photometrically and spectroscopically very similar to main-sequence stars are very common, and have luminosities which are a fraction of a magnitude to a couple of magnitudes above the main sequence at a given color. Since the derived density goes as the inverse luminosity of the population, an admixture of these class IV stars will lower the derived density. The K giants are in some senses even worse. One knows that stars from many populations funnel into the K giant region, and that the luminosity at a given color is very metallicity dependent. If there were, for instance, a strong inverse metallicity-height correlation, which there may well be, the effect would be that more distant stars would be more luminous and the density would be overestimated. One needs quantitative narrow-band photometry to investigate both of these effects in the samples used; that is hardly a difficult job today and needs badly to be done.

2. HOW MUCH IS THERE?

The total amount of dark matter associated with luminous matter, in galaxies, binary galaxies, groups, clusters, and stuctures like the Local Supercluster is in principle amenable to determination by dynamical investigations. Such investigations have, of course, been in progress by many workers for some years now, and the answers seem now to be converging, a phenomenon which is comforting but should not necessarily inspire confidence in the result. Marc Davis reviewed this subject ably here; it would appear from many independent lines of argument that Ω is about 0.2 under the assumption that the matter on scales larger than galaxies is distributed like galaxies are. For most of the arguments that go into determining that value the uncertain Hubble constant drops out. As interesting or perhaps more so is the question of the ratio of luminous to dark matter. The determination of that quantity and its possible variation from system to system and from one kind of system to another depends upon knowing mass-to-light ratios well as a function of color and population type. There seems no longer to be any strong evidence for variation with scale, or, indeed, any real variation at all once one is speaking of scales larger than the main bodies of galaxies. Values of $f=\rho_{\ell}/\rho_d$ of .05-.07 are obtained from arguments ranging from the dynamics of the Local Group to binary galaxies to the dynamics of other small groups and clusters to "global" measurements which in fact are only superpositions of the aforementioned results. The poster by Quinn here suggests that the ratio is about 0.1 for elliptical galaxies based on the behavior of the Malin-Carter shells. It would appear that a ratio f of 0.07 is unlikely to be wrong by a factor of two. In most formation scenarios this ratio should be universal; separation of dark and visible matter should occur only on small scales where dissipation can occur on the timescales of interest. (It should be noted that this situation need not prevail in the neutrino pancake picture and would not be expected to prevail in explosive scenarios like that of Ostriker and collaborators.) In biassing schemes, which are almost certainly necessary if one is to believe that Ω is unity, galaxies are not formed efficiently in low-density regions, but if the dark matter is primordial the ratio of matter which would have become the luminous matter in galaxies to the dark stuff should presumably be the same number. The constancy of f over observed systems may eventually, with somewhat better data than we possess currently, put strong constraints on biassed galaxy formation. Peebles has here and elsewhere strongly stressed the point that even if the biassing is a strict threshold phenomenon, heirarchical clustering will cause some mixing of the stuff in which no galaxy formation has occurred with that in which it has, giving apparent abnormally low values of f there. This phenomenon has not really been addressed in any of the n-body studies to date in which crude biassing schemes have been used.

It is worth at this point screaming about something about which it will do no good to scream, viz. the poor state of our knowledge of the Hubble constant. While it does fortuitously drop out or nearly drop out of the Ω determinations via dynamics, for almost all other questions it is of crucial importance. All inferences about the present universe from nucleosynthesis, from the processing of the perturbation spectrum through the early universe, from relic blackbody background fluctuations, depend sensitively on its value, and our ignorance of its value is the limiting factor in the application of many of these arguments. The obvious exhortation is clear: go do a better job.

3. WHERE IS IT?

That is, what is its distribution relative to that of luminous matter? As we have argued earlier, the existence of local dark matter distributed more-or-less like the stars in the disk would argue for dissipation in that component. An amusing possibility is the existence of a shadow galaxy made of shadow matter coincident with our own; if the physics of the shadow world were exactly like that in our own, this would be likely if not inevitable, and would neatly explain the required factor of two. The constraints on primordial helium production, however, rule out this possibility, since there would always be as much shadow stuff as "real" stuff, and the universe would expand too rapidly through the nucleosynthesis era, in precisely the same way it would if there were too many families of leptons with their associated neutrinos.

In galaxies, we know now that the rotation curves at large radii are dominated by dark material, a point made clear here by van der Kruit, Rubin, and Sancisi. The striking demonstration by Kalnajs four years ago at Besançon that rotation curves can be explained by constant M/L disks seems not to be tenable when applied to galaxies with modern rotation curves which extend to very large radii. The demonstrations we have heard here from Freeman and from Kormendy that dwarf disk systems also have dark halos is very important, and may, through the phase-space constraints developed by Tremaine and myself, eventually rule out neutrinos of a few tens of ev as candidates for the dark matter. A very interesting question discussed here is the dynamical importance of dark matter in the inner parts of galaxies. Since one does not know a priori the distribution of dark matter, there are no "maximal halo" models without a lower bound on the M/L for the disk material. The Galaxy is the only system for which we have direct evidence on this question, but the uncertainty in the disk mass is at least a factor of two and perhaps more. Here the disk is certainly a major contributor to the rotation curve interior to the sun, but it may not dominate. An exciting recent development is the Athanassoula-Bosma work on the multiplicity of the spiral pattern as influenced by the halo-to-disk mass ratio; minimum disk masses can in principle be determined by their technique.

We may eventually know the answer to this by the existence of rotation curves which can clearly not be explained in their inner parts by any reasonable M/L value for the visible mass, but in all the cases discussed so far there are reasonable doubts about the distribution of the gas (in edge-on galaxies with 21-cm rotation curves) and the existience of noncircular motions (in optical measurements in systems like NGC5194). If it turns out to be the way that current measurments suggest, i.e., that some galaxies require dark matter in their centers and others do not, a ready explanation may be found in the varying clumpiness of the initial perturbation. All workers who have investigated the effects of initial clumpiness have found that clumpy initial conditions lead to deVaucouleurs-law like systems with strong central concentrations, while smoother initial distributions result in less centrally condensed final configurations. Thus most galaxies have deVaucouleurs bulges, and a few, those with especially messy inital conditions, might have deVaucouleurs halos.

It would appear, then, that all big galaxies have halos, the evidence for spirals coming directly from the rotation curves, that for ellipticals in rather more indirect fashiion from group M/L's, shell geometry, and, for some, rather directly from X-ray data. A strong possibility exists that all little galaxies also have halos; certainly some do, as we have seen here.

The evidence from binary galaxies concerning the existence of dark matter is quite compelling, but the evidence on its distribution is very confusing. It would appear from current data and their interpretation that the total masses of binary systems are weakly or not at all correlated with their luminosities. It is yet unclear how uncertainties and systematic selection (physical as well as observational) for orbital eccentricities and phases influence these results, but if taken at face value, they imply that f is highly variable on the scale of individual galaxies. These results may or may not be related to the apparent large dispersion in M/L for small groups, which seemed some years ago to be adequately explained by the work of Gott and Turner in terms of a combination of contamination and statistical fluctuations in the virial ratio for small systems. It is an extremely important result if true, simply because it says that there can be variations, whatever the mechanism; biassing demands such variations systematically with density. Thus the study of binary galaxies would seem to bear importantly on this crucial issue.

On yet larger scales, from cluster dynamics, local group infall, and the correlation structure in velocity space, there is unequivocal evidence for large amounts of dark matter, and again there seems to be slow convergence in the consensus from these data to a value of Ω in the vicinity of 0.2.

What then, are we to do if our prejudices demand that Ω be unity? In this connection it should be noted that not all inflationary scenarios demand simultaneous flatness and homogeneity, and that at least one primordial inflationary model, that of Gott, produces negatively curved homogeneous models naturally. It may be that the currently fashionable GUT phase transition models which demand that $k=0$ if the universe has inflated enough to be reasonably homogeneous are not correct, or that quantum effects earlier had already established homogeneity prior to the GUT inflation.

If Ω must be unity, it would appear that most of the mass cannot be where most of the galaxies are. Where, then, is it? The presumption of the biassing picture is that the efficiency of galaxy formation is high in high-density regions and low in low-density regions. In great clusters the gas mass is of the same order as the stellar mass (though there is some dispute about that) so the efficiency is high, of order unity, in dense regions. The constancy of f in smaller systems argues that the biassing is not a strong function of density when the density is high, and a simple picture in which there is a threshold below which the efficiency is low or zero, and above which it is high, suggests itself. It is important to note that essentially all our information about dynamics comes from places where the galaxy density is at least an order of magnitude above the mean, except for the local supercluster infall, in which case we deal with a density only about three times the mean. The fact that f is of the same order in the local supercluster as it is in very much denser structures is a little disconcerting. If the supercluster kinematics could be traced with high accuracy to greater distances and lower densities yet, severe limits might be placed on biassing; the data, however, will be long in coming.

One possibility which may or may not be relevant, but may again bear on the biassing quesion, is whether the dark matter exists in large black lumps of galaxy or even higher mass. This does not by itself settle the question of where it is, because one must still arrange for the lumps not to associate with galaxies, but their existence would clearly enhance the biassing cause: collapsed structures of dark matter without associated luminous mass are earmarks of all the biassing schemes discussed so far. Clues about this matter may be crying for attention in most of the gravitational lens systems so far discovered, in which no plausible lens galaxies are found even in quite sensitive searches. Even the beautiful data Tyson showed us for Q2345 may turn out to be the best case; the galaxy between the images is so faint/and/or distant that it is very implausible that it can be the lens.

The distribution of mass on very large scales thus holds the key to the crucial connection between dark matter and cosmology, however it may be distributed on the scales of galaxies and clusters. Almost the only handle we have on this is the dipole anisotropy in the microwave background, which we believe must arise from our peculiar velocity, which must in turn have arisen from peculiar gravitational acceleration due to the inhomogeneous distribution of mass in our neighborhood. We have heard here both from Yahil and Davis of the application of Gott's luminosity/force technique to the IRAS catalog, and the derivation thereby of fairly large Ω, implying that the mass is distributed much more smoothly than the light. These results are very interesting, but clearly some caution must be exercised in their interpretation. The derived M/L's of the IRAS sources vary enormously, over at least a couple of orders of magnitude, whereas optical M/L's vary probably over no more than a factor of 5 or so (VISIBLE mass/light) Thus slight environmental biasses in the M/L's can produce any effect one wants, and it would be surprising to me if there were not in fact fairly strong such effects.

Another matter related to the large-scale distribution is the question of the cluster-cluster correlation function, the scale of which has profound consequences for currently fashionable ideas about the nature of the dark matter, but we will defer the discussion until we take up that question, which we do next.

4. WHAT IS IT?

First of all, is there more than one kind? There is, I think, no convincing evidence on this point as yet. If the dark material in the disk of the Galaxy cannot be explained by astrophysical processes, or if global cosmological tests indicate that the universe is currently radiation-dominated (both of which I find unlikely), then the question will have to be faced squarely, but now I think we cannot intelligently address the question.

The current fashion in certainly to ascribe the dark matter to some new stable or nearly stable neutral particle, among which the favorites are GUT axions and the lightest supersymmetric partner to ordinary particles, probably the photino or gravitino, but perhaps something more exotic. The current state of this rapidly evolving scene was reviewed beautifully here by Mike Turner. It would be very surprising if it stabilizes anytime soon.

There are arguments for baryonic dark matter beyond the wish not to invent arbitrary solutions to our problems. There is evidence that the Population II mass function is very steep in the halo field as well as in massive globular clusters, and an extension at the low-mass end to quite plausible masses leads to very large mass-to-light ratios (and, incidentally, to very low heavy-element yields). A picture in which the low-mass cutoff progresses smoothly from masses like 0.1 solar mass to perhaps 10^{-3} solar masses as one goes from the central regions of a galaxy out makes a qualitatively plausible model which explains rotation curves quite handily. It entails no mystery as to why the amount of dark matter is within an order of magnitude or so of the amount of visible matter, and at least makes plausible the observed fact that rotation curves are flatish from regions where galaxies are almost certainly dominated by visible mass out to regions in which they are clearly dominated by dark matter. There is evidence from cooling flows, as we have heard, that such baryonic dark matter is being formed by some process before our eyes in a few particularly spectacular flows, and may be a general feature of such flows. It does seem a little strange that any such "extended bulge" halo population produces dark halos which seem to be completely uncorrelated with the amount of ordinary bulge population--it is true that large-bulge systems have, on average, higher rotation velocities, but large Sc's can have extensive halos and no bulge at all.

There are, of course, serious problems with baryonic dark matter, some of which were discussed here and others not. Understanding the absence of microwave background fluctuations on arcminute scales is very difficult whether the perturbations are adiabatic or isocurvature, and probably can be understood only if there is reionization, with its attendant energy problems. Primordial nucleosynthesis can be understood only if $\Omega\ h^{-2}$ is small, of order a few hundredths; the exact upper bound is a matter of some controversy, as we have heard discussed. It is unlikely that Ω is much smaller than its currently fashionable dynamical value, about 0.2, so a reliable value for the Hubble constant would have a large impact upon this question. With Ω of 0.2, the reionization must occur earlier than $z=20$, probably considerably earlier (for $H=50$, which probably is near the maximum value consistent with the nucleosynthesis data, the number is 32; these are all for unit scattering optical depth, and that is almost certainly insufficient--more realistic redshifts are about 1.6 times larger, corresponding to TAUs of about two. Thus 50 is a realistic minimum redshift). The fraction of matter now in known structures dense enough to have been formed then is very small, and furthermore the Compton cooling is very efficient, so it is not at all clear whether reionization can reasonably occur (and survive). We certainly know of no energy sources at all at those epochs, but that is probably irrelevant. It is worth noting that if the dark matter is like that claimed to be forming in the cooling flows at present, it is not a candidate for the ionizing energy, both because it emits none and because dynamical arguments rule out most of it having formed so early.

Rough estimates place the energy requirements at about 1 keV per nucleon to ionize and stay ionized long enough to create the desired optical depth, with perhaps one percent of the matter contributing energy, or 100 Kev per contributing nucleon in ionizing energy. These figures are not much in excess of supernova energies, but are far in excess of net supernova output per nucleon in stars with current mass functions. Thus something exotic will almost certainly have to be invoked to do the ionizing--but the energy requirements per se are not unreasonable.

Thus it might just be possible to have baryonic dark matter and not give up primordial nucleosynthesis. The notion is not outrageous, as Bernard Carr attempted to persuade us, but there are difficulties.

What about neutral particles? The first question is why f is about .07 and not a trillion or a trillionth. The coincidence is not actually so startling except perhaps for the axions, since one has no a priori notion of where the Peccei-Quinn symmetry should be broken. The expected masses for the neutrinos (if any) are of the correct order but too small by a few orders of magnitude, though perhaps the neutrinos belonging to the heaviest leptons should be heavier. The mass (and indeed the identity) of the lightest supersymmetric particle is likewise very uncertain, but favorite values are a few GeV, and if it interacts weakly, the mean density is about that required, as shown some time ago by Lee[1] and Weinberg.

The advantages of nonbaryonic dark matter for nucleosynthesis and for reducing the amplitude of the background fluctuations are well known; the former was discussed here in detail by Audouze, the latter unfortunately not discussed, though there is excellent recent published work by Vittorio and Silk[2,3] and by Bond and Efstathiou[4].

It is perhaps worth discussing briefly the matter of the flat rotation curves under the supposition that the dark matter is nonbaryonic. I think that the phenomenon is due to concidence, but one we are presented with entirely independently. It has been known for a long time, first from Peebles' analytical work and later from a long series of numerical experiments, that the mean value of the angular momentum parameter $\Lambda = J|E|^{1/2}M^{-5/2}G^{-1}$, which for a system in virial equilibrium is roughly the ratio of the mean rotation velocity to the total dynamical velocity, is about 0.07. I have argued that f, the ratio of visible to dark mass, is of that same order. For a disk, Λ is of order unity, and if the baryonic component cools and sinks in the roughly $1/r^{-2}$ halo of dark stuff until it is rotationally supported, it must thus contract by a factor of roughly the reciprocal of its original Λ, or about 15. It thus becomes 15^3 times denser. The dark halo at the new radius is 15^2 times denser, and since the original baryon density was $f = 1/15$ times the total dark density, it is now at about the same density as the dark material. Thus the baryons should not either swamp or be swamped by the dark material at their final equilibrium place, but should be comparable in dynamical importance. It is thus not surprising that the rotation curves are nearly flat;

detailed models by Ryden and myself, outlined in a poster here, show that the idea works in detail.

If the dark matter is nonbaryonic, the inevitable question of whether it is hot or cold, in the sense of whether its phase space density is much lower than that in galactic halos (hot) or much higher (cold). The original elementary particle candidate for the dark matter was of course some heavy neutrino, of order 100 eV in mass, which is a hot particle. From the very beginning considerable doubt was raised as to whether such particles could partake of the known structure in the universe on the scale of galaxies, both on account of their phase-space densities, which in the coarse-grained sense can only decrease in the absence of dissipation, and the even more serious trouble brought about by the free-streaming erasure of small-scale perturbations early. The recent simulations by Davis, Efstathiou, Frenk, and White and described here by Davis illustrate the difficulty very strikingly. I think it very unlikely that the dark matter is neutrinos, but the unequivocal measurement of a neutrino mass would, of course, cause a quick retreat from this view.

We have on the other hand the almost uncanny success of the cold dark matter picture, documented most vividly in the work of Blumenthal et al.[5]; its appeal is obvious from the sheer number of poster papers here dealing with various aspects of the scenario. The spectrum, the shape (but not yet the amplitude) of which can be understood on very general grounds, accounts well for galaxy and cluster-sized structures when the amplitude is normalized to fit the galaxy distribution on scales of a few megaparsecs. Even the shapes of the rotation curves of galaxies seem to be predicted. There is now considerable confusion about the normalization, however, since it is clear that either Ω is less than one, in which case it is not clear that the assumptions which lead to the cold dark matter spectrum are valid (inflation), or that there is biassing, in which case the amplitude of the matter fluctuations now must be smaller than those in the galaxy distribution by a factor of two or three at the normalizing scales. There is in addition the difficulty that the cold dark matter correlation function goes negative at about 20 $(\Omega h^2)^{-1}$ Mpc, and no positive correlations would be expected on larger scales. There is, of course, the suggestion that the cluster-cluster correlation function is significantly nonzero and positive to larger distances, perhaps as large as 100 Mpc. How seriously this should be taken is a matter of some debate; certainly the catalogs from which these data come are not satisfactory statistically, but it will be some years before properly objectively prepared catalogs are available. There is, of course, the related observation of holes larger than the correlation length. It is in my opinion not completely clear yet what one expects with a given fluctuation spectrum; the largest numerical experiments yet run are not large enough to address that question with any certainty.

5. POSTLOGUE

There is the exciting possibility that the dark matter particle will be discovered, either in accelerator experiments or in clever directdetection experiments; the discovery of a stable weakly-interacting fermion in the relevant mass range (a few GeV), or the confirmation of the existence of the axion, or (heaven forbid) the unequivocal measurement of a neutrino mass would make it difficult not to take nonbaryonic dark matter seriously. It seems unlikely that any of these things will happen soon.

It also seems unlikely that there will be an accurate, agreed-upon value for the Hubble constant very soon, which in my opinion is the single thing we need most at this point to constrain theoretical flight. Other important observational material will also be long in coming: to MEASURE finally a galaxy- and cluster-scale perturbation in the background; to have an objectively constructed galaxy and cluster catalog with well-planned redshift coverage to look at random velocities and large-scale correlations, just to name a couple. I think that we will be debating these same issues for a long while to come, and I do not look forward to decisive answers in the next few years. We have, I think, more-or-less agreed to the existence of dark matter and its importance in understanding the origin and dynamics of structure in the univese. An important first step, but there will be very many more to go.

REFERENCES (to work cited in the summary which was not discussed fully at the conference)

1. Lee, B. and Weinberg, S., Phys. Rev. Letters 38, 1237 (1977).
2. Vittorio, N., and Silk, J., Ap. J. (Letters) 285, L39 (1984).
3. ibid, Ap. J. (Letters) 293, L1 (1985).
4. Bond, J. and Efstathiou, G., Ap. J. (Letters) 285, L45 (1984).
5. Blumenthal, G., Faber, S., Primack, J., and Rees, M., Nature 311, 587 (1984).